

# The Empire Is Dead, Long Live the Empire!

## Long-Run Persistence of Trust and Corruption in the Bureaucracy<sup>\*</sup>

The Empire Is Dead, Long Live the Empire

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We hypothesize that the Habsburg Empire with its well-respected administration increased citizens' trust in local public services. In several Eastern European countries, communities on both sides of the long-gone Habsburg border have shared common formal institutions for a century now. We use a border specification and a two-dimensional geographic regression discontinuity design to identify from

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individuals living within a restricted band around the former border. We find that historical Habsburg affiliation increases current trust and reduces corruption in courts and police. Falsification tests of exogenously moved borders, geographic and pre-existing differences, and interpersonal trust corroborate a genuine Habsburg effect.

*“No other family has endured so long or left so deep a mark upon Europe: the Habsburgs were the greatest dynasty of modern history, and the history of central Europe revolves around them, not they around it.”*

AJP Taylor (1948, p.12),

*The Habsburg Monarchy 1809-1918*

A large literature suggests that institutions matter for the functioning and development of an economy. What happens to these institutions when the state with which they are associated ceases to exist? The famous phrase “The emperor is dead, long live the emperor!” indicates that, even though individual emperors may die, their empire lives on with their immediate successors. In this paper, we show that even if whole empires themselves perish, they can leave a lasting legacy in cultural norms and the ensuing functioning of state institutions several generations after their formal institutions have ceased to exist. Specifically, we find that the Habsburg Empire, which went down in 1918, still affects trust and corruption in local public services in Central and Eastern Europe today. Our findings add to a growing literature indicating that history can have long-lasting effects (cf. Nunn, 2009, 2014; Spolaore and

Wacziarg, 2013) through its impact on current formal institutions<sup>1</sup> or on values, beliefs, and cultural norms.<sup>2</sup> Our results indicate that long-gone formal institutions can have a persistent impact on cultural norms of social behavior, thereby affecting the functioning of interactions between citizens and the state.

This finding is important for our understanding of the origins and maintenance of collective action in general, because trust in the key institutions of the state and their proper functioning is crucial in facilitating collective action (Ostrom, 1998). Trust among participants (Arrow, 1972) and the enforcement of rules and property rights (North, 1990) are two leading mechanisms that help to organize human cooperation, interaction, and exchange, thereby providing the cultural and legal underpinning for groups to achieve mutually productive outcomes. As views on the optimal scope of the state differ, we focus our analysis on two public services that enact basic functions of the state which even critical skeptics of government intervention support: the courts and the police as the enforcers of rules in collective action (e.g., Hayek, 1960). Rather than analyzing interpersonal trust, we focus on citizens' trust in these state institutions and on corruption, a leading example of malfunctioning of the interaction of citizens with the state.

The Habsburg Empire is historically known as a multi-ethnic state with a relatively well-functioning, respected bureaucracy.<sup>3</sup> Historians characterize the Habsburg bureaucracy as “fairly honest, quite hard-working, and generally high-minded” (Taylor, 1948, p. 44) – in contrast to the Ottoman and Russian Empires in Eastern Europe (Ingrao, 1996, 2000; Subtelny, 2007). We argue that this attitude created trust in its inhabitants in the respectability of government institutions, with ensuing effects on the functioning of citizen-state interactions, particularly at the local level. This cultural norm survived even the oppressive rules of the socialist system and is still alive today. However, the formal institutions of the

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<sup>1</sup> E.g., North (1990); Engerman and Sokoloff (1997); La Porta *et al.* (1998); Acemoglu *et al.* (2001).

<sup>2</sup> E.g., Putnam (1993); Greif (1994); Alesina and Fuchs-Schündeln (2007); Guiso *et al.* (2008a); Tabellini (2010).

<sup>3</sup> When referring to the Habsburg Empire in this paper, we focus on its impact in Eastern Europe and do not refer to Habsburg influence in Western Europe such as in Spain and the Netherlands.

empire ceased to exist with the collapse of the Habsburg Empire after World War I, breaking up into separate nation states that have seen several waves of drastic institutional changes since.

To test whether the cultural norms originating in the Habsburg Empire still endure today, we use the micro dataset of the 2006 Life in Transition Survey (LiTS) that provides measures of trust and corruption in Eastern European countries. We focus on the 17 countries that comprise the successor states of the Habsburg Empire and their neighboring countries. Drawing on a variety of historical sources, we coded the location of each observation in the LiTS dataset in terms of its affiliation with the Habsburg Empire. Yet, a simple comparison of cultural measures across countries with diverse populations, geographies, and intervening experiences may easily be biased by unobserved heterogeneity.

To identify the enduring effect of the Habsburg Empire, we therefore employ two types of specifications that exploit the within-country variation created by the Habsburg Empire in Eastern Europe. The first specification, which we refer to as border specification, compares individuals living in communities located within 200 kilometers of each other on either side of the long-gone Habsburg border. In order not to capture unobserved country heterogeneity, we use country fixed effects to restrict the analysis strictly to variation within individual modern-day countries. This identification exploits the fact that the former Habsburg border cuts straight through five countries today – Montenegro, Poland, Romania, Serbia, and Ukraine. Communities on the two sides of the former border have been sharing a common statehood for generations now. Additionally, we control for a large set of individual-level factors such as education, religion, language, wealth indicators, and urbanity.

The second type of specification adopts geographic regression discontinuity designs (RDD), both of the one-dimensional kind that controls for polynomials in distance to border and of the two-dimensional kind proposed by Dell (2010) that controls for polynomials in latitude and longitude. We also reduce the size of the bandwidth around the border down to 25 km. While the RDD may provide cleaner

identification with sharp geographic discontinuities, in our setting of effects of long-gone borders rather than short-run immediate policy effects it may also be more prone to attenuation from diffusion and interaction effects across the border.

Results from both specifications suggest that by establishing cultural norms, the Habsburg Empire still affects the interactions of humans with their state institutions today. Comparing individuals left and right of the long-gone Habsburg border, people living in locations that used to be territory of the Habsburg Empire have higher trust in courts and police. These trust differentials also transform into real differences in the extent to which bribes have to be paid for these local public services. The most robust finding is a negative effect of former Habsburg affiliation on bribes to courts. By contrast, the negative effect on bribes to road police gets smaller and insignificant, and the effects on trust lose statistical significance, in more demanding specifications with smaller bandwidths or higher-order polynomials.

Evidence from a firm dataset, the Business Environment and Enterprise Performance Survey (BEEPS), corroborates the general pattern of results derived from the LiTS household dataset and underscores the economic significance of the findings.

A set of falsification tests validates a causal interpretation of the results and rules out other channels of historical influence. First, when using “placebo” borders 100 kilometers inwards or outwards of the actual Habsburg border, we do not find any effects. This indicates that our results capture a specific Habsburg effect, rather than a general West-East pattern. Second, we verify that altitude does not vary significantly between the two sides of the former Habsburg border, thereby excluding obvious geographical differences between the Habsburg and non-Habsburg sample. Third, we do not find significant differences between the two sides in terms of medieval city size, access to medieval trade routes, and presence of a medieval diocesan town, suggesting that the Habsburg effect is not simply a perpetuation of differences that existed before Habsburg influence. While the available data on pre-

existing differences is clearly limited, as a fourth falsification test we show that there is no Habsburg effect on trust in other people and on membership in civic organizations. Thus, the identified Habsburg effect is genuine to citizen-state interactions and does not perpetuate a general pre-existing pattern of trust.

The remainder of the paper is organized as follows. Section 1 provides theoretical and historical background and derives the main hypotheses to be tested. Section 2 describes our data. Section 3 develops the empirical identification strategy. Section 4 presents our basic results of Habsburg effects on trust and corruption in local public services. Section 5 reports a series of falsification tests to support the validity of the identification strategy. Section 6 presents supporting evidence from a business survey. Section 7 concludes. Additional aspects of the Habsburg effect are analyzed in an Appendix.

## **1 Theoretical and Historical Background**

This section starts out by discussing different channels through which history may leave a legacy for current outcomes and relates these to the existing literature. Next is a brief overview of the history of the Habsburg Empire as it relates to the subject of our analysis. From this theoretical and historical background, we derive the main hypotheses to be tested in the paper.

### *1.1 Why History Matters: Some Theory, with Reference to Related Literature*

A growing literature investigates the different mechanisms leading to the fact that history often has long-term repercussions for economic development (see Nunn, 2009, 2014; Spolaore and Wacziarg, 2013 for reviews). First, historical circumstances and events can shape the state and evolution of *formal institutions* that survive and affect economic interactions and outcomes today (e.g., North, 1990). For example, recent research on the importance of colonial rule for long-term economic development

emphasizes its impact on current formal institutions.<sup>4</sup> A second reason why past developments may be related to current developments is *geography*. Geographical and ecological factors that do not vary over time, such as climate zone, disease environment, natural endowments, coastal location, and continental orientation may have direct effects on economic development past and present, as well as on the path of history (Diamond, 1997; Sachs, 2003).<sup>5</sup> Third, historical events may have caused differential development of people's knowledge, *human capital*, which subsequently led to differences in economic development. For example, Glaeser *et al.* (2004) argue that European colonization may have left a long-term legacy not because of institutional development, but because colonial settlers brought their human capital with them.<sup>6</sup>

The fourth mechanism through which history can affect long-term outcomes is the topic of this paper: History can matter for later economic development by affecting people's *cultural norms and values* which then persist over time.<sup>7</sup> Recent literature shows that cultural factors such as trust, manners of social interaction, and other values, beliefs, and norms have important repercussions for economic development (e.g., Algan and Cahuc, 2010; Tabellini, 2010). Probably the best-known example of the cultural channel

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<sup>4</sup> This effect may work through the bearing of large-scale plantation production on inequality and thus institutional development (Engerman and Sokoloff, 1997), through the introduction of civil vs. common law legal systems (La Porta *et al.* 1997) or through persistence of property-rights institutions determined by initial disease environments (Acemoglu, Johnson, and Robinson 2001). Similarly, Acemoglu *et al.* (2005) argue that access to Atlantic trade affected the evolution of formal institutions in Western Europe. Nunn (2008) shows that external trade in slaves had long-run repercussions in Africa. Jha (2013) argues that medieval trade access led to institutions promoting religious tolerance in India. Acemoglu *et al.* (2011) show that French invasion of Central Europe after the French Revolution brought radical institutional changes that left a long-lasting mark.

<sup>5</sup> The effect of geography may interact with the evolution of formal institutions, for example, when the geographical endowment spurs cash cropping (Engerman and Sokoloff, 1997) or when disease environments affect institutional choices because they impact settler mortality (Acemoglu *et al.*, 2001).

<sup>6</sup> As another example, Becker and Woessmann (2009) show that the Protestant Reformation affected later economic development within Prussia and across countries by raising literacy levels, required to read the bible. In a similar vein, Woodberry (2004) depicts a positive association between historic Protestant missionaries and modern-day school enrollment across colonized countries. Akçomak *et al.* (2014) show that a religious community founded in the 14<sup>th</sup> century is linked to literacy rates and subsequent city growth in the Netherlands.

<sup>7</sup> Ashraf and Galor (2013) document the longest historical legacy so far in the hump-shaped relationship between human genetic diversity emerging from prehistoric migration patterns and current economic development.

is Weber's (1904) hypothesis that a specific Protestant work ethic furthered capitalist development.<sup>8</sup>

Puham (1993) conjectures that the culture of independence fostered by the experience of free city-states at the turn of the first millennium fostered a culture of independence that left a mark on social capital and economic development in Italy today. Guiso *et al.* (2008a) find supportive evidence.<sup>9</sup> Alesina and Fuchs-Schündeln (2007) document that individuals who were exposed to different political and institutional systems in West and East Germany from 1945-1990 have different preferences for redistribution. Our study indicates that cultural effects can persist over generations.

Cultural evolution may also be closely interrelated with institutional development (Greif, 2006; Greif and Tabellini, 2010).<sup>10</sup> With respect to empires, evidence in Grosjean (2011a) suggests that in Eastern Europe, people living in areas that used to be part of the same empire have more similar trust values.<sup>11</sup>

Differing cultural norms can affect outcomes even under the same formal institutions, for example when they lead to a different functioning of the formal institutions.

The mechanisms of the transmission of cultural norms over time are not well understood so far. Most of the current literature focuses on intergenerational transmission within families,<sup>12</sup> for example when looking at parent-child correlations (Dohmen *et al.*, 2012) or when identifying links of U.S. immigrants with their own or their ancestors' home countries.<sup>13</sup> Alternatively, cultural norms can persist by

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<sup>8</sup> However, Becker and Woessmann (2009) and Cantoni (2014) find little evidence for this specific channel in historical indicators of economic development.

<sup>9</sup> As a classical example of the cultural channel, Greif (1994) describes how the collectivist vs. individual attitudes towards contract enforcement of Maghribi vs. Genoese medieval merchants affected their subsequent developments. More recently, Grosfeld *et al.* (2013) find that regions in the former Russian Empire show higher anti-market attitudes today if they faced ethnic hostility against Jews and solidarity among the non-Jewish population in the past.

<sup>10</sup> Nunn and Wantchekon (2011) identify cultural norms and formal institutions as two separate channels of the effect of slave trade in Africa. Fisman and Miguel (2007) find that both cultural norms of the home country and legal enforcement mechanisms affect parking-violation behavior of United Nations officials in Manhattan.

<sup>11</sup> Cultural norms may also relate to historical education. Tabellini (2010) finds that historical measures of literacy and political institutions are correlated with cultural variables today and subsequently with economic development across European regions.

<sup>12</sup> E.g., Bisin and Verdier (2000); Tabellini (2008b); Guiso *et al.* (2008b).

<sup>13</sup> E.g., Guiso *et al.* (2006); Giuliano (2007); Tabellini (2008a); Fernández and Fogli (2009); Algan and Cahuc (2010).



transmission via schooling, or even through the very nature of local interactions: If a person with a prior of trust in local public services moves into a town with a corrupt bureaucracy, he might quickly change over to a prior of distrust, and vice versa.<sup>14</sup> Thus, through continuous reciprocal behavior, a local equilibrium of trust or distrust may be maintained even with migration, and even without any family ties. Despite a multiplicity of possible cultural equilibria, the selected equilibrium can thus be highly persistent (cf. Guiso *et al.*, 2008b; Belloc and Bowles, 2009). In line with this argument, Ichino and Maggi (2000) show that workers moving into other branches of a large Italian firm in fact adopt local habits of their new region quickly. Similarly, the large literature on peer effects suggests that people's attributes may affect other people (see Sacerdote, 2011 for a survey).

Much of the work on history's role through cultural persistence has, in the tradition of Arrow (1972), focused on trust among people in interpersonal interactions. Little is known, however, on the effect of historical tradition on people's trust in a state and the ensuing functioning of state institutions. Such trust in citizen-state interactions gains particular relevance from the importance of rule enforcement in collective action and a well-functioning state (see above). Trust in and trustworthiness of state institutions is also the more obvious way in which historical empires with the particular character of their bureaucracies may leave lasting legacies.

In this paper, we aim to test the fourth channel of history on cultural norms today, while controlling for the other channels, in the specific case of the Habsburg Empire. Specifically, we are interested in whether persistent cultural norms towards the interaction of individuals and local state authorities can be a channel through which historical, but long-gone formal institutions can affect outcomes today. Beyond

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<sup>14</sup> The idea that trust priors adopt quickly to local environments is consistent with the theory of broken windows which suggests that first impressions of local environments affect lawful behavior. It goes back to an experiment showing that an abandoned open car was vandalized quickly in the Bronx but not in Palo Alto, where it was also vandalized as soon as it had a broken window. The effectiveness of broken windows policy, which influenced anti-crime policies in such cities as New York City and Los Angeles, has been validated in Corman and Mocan (2005), and laboratory experiments confirm that first impressions causally affect social behavior (Engel *et al.*, 2014).

trust, we also analyze whether the effect extends to corruption – defined as the misuse of public office for private gains (Svensson, 2005) – as a real consequence in the interaction between citizens and the state.

In a related insightful paper, Grosjean (2011a) uses data from 21 LiTS countries to estimate a gravity model that regresses differences in interpersonal trust between pairs of locations on an indicator of shared membership in the same empire.<sup>15</sup> She finds that having been in the same empire for more than a century, and particularly for more than four centuries, is associated with reduced distance in trust in other people. While also addressing the topic of empires and trust, our more specific focus on the legacy of the Habsburg Empire, the resulting focus on trust in the bureaucracy as opposed to trust in other people, individual-level analysis, and our identification in border specifications that aim to address more directly any issues of bias from unobserved heterogeneity provide new insights into the particularities of the effect of empires on trust.<sup>16,17</sup>

## 1.2 Historical Background on the Habsburg Empire

Beginning in the 11<sup>th</sup> century, the Habsburg dynasty gained a multitude of territories from Spain in the West to Galicia in the East. The Habsburg<sup>18</sup> identification with Austria began when Rudolf IV of

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<sup>15</sup> Similarly, Mitchener and Weidenmier (2008) show in a gravity model that belonging to the same empire is related to trade patterns.

<sup>16</sup> Two other excellent papers using LiTS data address quite different topics, but are actually closer to our paper in terms of identification (note that all three papers emerged contemporaneously to our work and that we became aware of them only after the first several presentations of our initial paper version). Grosjean (2011b) uses country fixed effects in a six-country sample to estimate the effect of historical Ottoman rule on current financial development, measured by the fraction of households in a location who use a bank account or credit card, as well as on real-sector indicators. She finds effects on financial but not economic development (in Appendix B.1, we distinguish our Habsburg effect from any possible Ottoman effect in our topic of analysis). Grosjean and Senik (2011) use a model with country fixed effects in a 28-country sample to estimate the effect of an index of regional market liberalization on support for democracy and a spatial regression discontinuity design across modern country frontiers to estimate the effect of democracy on support for market economy. Despite the different topic, the latter identification strategy has some similarity with ours.

<sup>17</sup> Recently, Peisakhin (2012) studies effects of the partition between the Habsburg and Russian empires on political attitudes and behaviors of Ukrainians today, and Grosfeld and Zhuravskaya (2013) study effects of the partition of Poland among the Habsburg, Russian, and Prussian empires on current voting outcomes in Poland.

<sup>18</sup> For simplicity, we generally use the term *Habsburg Empire* to refer to the entire Habsburg history, although it is sometimes used more narrowly to refer to the period 1526-1867. The name *Austrian Empire* is officially applied during 1804-1867, and *Austro-Hungarian Empire* officially describes the two states with one common reign during 1867-1918. Our main

Habsburg was elected king of the Holy Roman Empire in 1273. Since then, the Habsburgs continuously expanded their territories, in the eastward direction that is relevant in our analysis mostly by wars – including wars that were waged without Austria. In the 16<sup>th</sup> century, more than half of Europe was ruled by the House of Habsburg. Charles V (1500-1558) ruled the Holy Roman Empire, a realm of almost four million square kilometers where “the sun never sets.” For five centuries, Austria was the great Central European superpower, until its dismemberment in World War I (Zöllner, 1990). Reasons underlying the ultimate fall of the Habsburg Empire in 1918 include national intentions of the elites of the different peoples living in the Habsburg territories and the political will of the winning powers of World War I.<sup>19</sup>

In Eastern Europe,<sup>20</sup> Habsburg broadened its territory in 1526, when Ferdinand of Austria, brother of Charles V, was elected King of Hungary, Croatia, and Bohemia. Habsburg now had to bear the main burden of the Ottoman drive from the Balkans into Central Europe. Twice, the expanding Ottoman Empire tried to capture Vienna, in 1529 and 1684. The latter battle marked the beginning of the political hegemony of the Habsburg dynasty in Eastern Europe.<sup>21</sup> Step by step, it conquered vast territories along the Danube – in Hungary, Croatia, Serbia, and Romania – constantly driving back the Ottomans. External events caused Habsburg’s north-eastward expansion: the First Partition of Poland in 1772, arranged by Russia and Prussia, brought Galicia and Lodomeria. The acquisition of Bukovina in 1775 was a side effect of the Treaty of Küçük Kainardca (1774) after the Russo-Ottoman War. Habsburg attempted to prevent Russia and its ally Serbia from gaining further territories in the area, until the conflict with Russia

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descriptions of the Habsburg bureaucracy mostly relate to the Austrian part of the Habsburg Empire. While we do not deny that there may be relevant differences between the Austrian and the Hungarian part, of the five countries in our main empirical analysis below, the Hungarian part was active in one country (Romania) only, and we show that results are not sensitive to dropping Romania from the analysis altogether. That is, our results are not sensitive to the Hungarian part of the Habsburg Empire.

<sup>19</sup> For historical background on different economic aspects of the Habsburg Empire, see, e.g., Good (1984), Komlos (1983), and Schulze and Wolf (2009, 2012).

<sup>20</sup> In the remainder of the paper, we use “Eastern Europe” as shorthand for those countries that used to be east of the “Iron Curtain,” which are the countries covered by the LiTS dataset used in our analysis.

<sup>21</sup> See also Iyigun (2008) on the history and repercussions of the Ottomans’ military engagement in Europe.

became notorious in the 19<sup>th</sup> century. To maintain a balance of power between the leading European powers, the Treaty of Berlin in 1878 allowed the Austro-Hungarian Empire to occupy Bosnia, Herzegovina, and the Sanjak of Novi Pazar in Serbia and Montenegro (Glenny, 2000).

The Habsburg Empire is the prototype of a *Vielvölkerstaat* (state composed of many peoples) that largely respected the identity and local differences of various parts of the empire (Ingrao, 2000). Despite the national aspirations of the different peoples, some aspects of Habsburg policy were widely accepted. In particular, the bureaucracy throughout the empire was well respected by the population because of its reliability (Sieghart, 1932).<sup>22</sup> Taylor (1948, p. 44) paraphrases this as follows: “The Austrian bureaucracy was fairly honest, quite hard-working, and generally high-minded, it probably did more good than harm.” Similarly, Magocsi (2010, p. 414) points out that “despite its diversity, the Habsburg Empire was not simply a motley conglomerate of territories and peoples”, but that “the bureaucracy, which became an efficient and fair, if sometimes overbearing, instrument of Austrian rule” and the “legal system” were among the uniting factors that “obtained until – and even after – the demise of the empire in 1918”.

Originally, the different parts of the Habsburg Empire were only loosely tied together. This changed during the 18<sup>th</sup> century when the organization was increasingly centralized, although most parts of the actual administration remained highly decentralized. Already Maria Theresa (1741-1765) began to establish a *Beamtenstaat* (an administration of civil servants) and instituted *Kreishauptmänner* (county governors) to supervise local administration in different parts of the empire. Her son, Josef II (1765-1790), an enlightened and secularized monarch, resolutely continued this way. He ended censorship, introduced complex legal reforms, established German as official language throughout the empire, promoted

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<sup>22</sup> Several elements of the Habsburg bureaucracy survive to this day. For example, Emperor Franz Joseph was known to get up early and expected to be able to reach his civil servants in office as well. In the Czech Republic, offices generally open at 7am to this day.

public education in the vernacular, and founded institutions of social and medical care.<sup>23</sup> As discussed in greater detail in Appendix B.1, this administration of the Habsburg Empire differed in central aspects from the neighboring empires. Compared to its neighbors to the east and north, the Habsburg state ruled in a manner that was more acceptable and predictable for the inhabitants of the empire.

It is hard to define a definite universal pattern of how Habsburg implemented its bureaucracy in its newly acquired Eastern European territories and how bureaucratic behaviors and expectations may have survived beyond the demise of the Habsburg Empire. However, there is a basic pattern that is general to most of the territories in our analysis below. Thus, when a new territory fell under Habsburg rule, the emperor quickly installed a governor who established a rich local bureaucracy that over time was filled with local people who had been sent for administrative training in Vienna. This established a Habsburg-trained local administration whose efficiency was appreciated to the extent that most of the well-trained bureaucrats were allowed to stay on in their jobs in the new administration after the Habsburg Empire broke down.

This pattern is quite evident in the case study of Lviv in Galicia (nowadays in Western Ukraine), the fourth-largest city in the Habsburg Empire. Soon after Lviv came to Habsburg with the First Partition of Poland in 1772, the Habsburgs terminated the Polish administrative structures and established a local government, administration, and judiciary with detailed administrative bodies whose local civil servants were trained in Habsburg administration (Mark, 1994). When, after the demise of the Habsburg Empire, Lviv came under Polish rule in 1918, the previous judicial system was maintained regionally in Galicia

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<sup>23</sup> Additional well-received aspects of Habsburg policy include transfers in the form of subsidies and infrastructure projects such as railroads to less developed regions in order to foster their integration. In the Habsburg lands, education was more important than in the Russian and Ottoman Empires. As an exemplification, in his famous novel “The Bridge over the Drina”, Literature Nobel Prize laureate Ivo Andrić describes how the Habsburg rule introduced public infrastructure, well-functioning administration, and public order and created an era of relative security and advancement when it took over Zvornik, a town in the east of Bosnia and Herzegovina near Serbia, in 1878. See also Imamović (2007) and Bencze (2006). In addition, the Austro-Hungarian army was a functioning multicultural microcosm and an important instrument for integrating peoples from all over the Habsburg territories.

(Kaczmarek, 2010), large parts of the previous administrators such as the Habsburg-trained local judiciary stayed on under the new Polish system because of their professional competencies (Kraft, 2002), and many Poles went on to study in Austria in the 1920s (Mroczka, 1996).<sup>24</sup> This may have allowed for a positive valuation of the administration to survive. Despite the drastic changes during World War II and afterwards under Soviet rule, the Habsburg legacy is clearly visible in the local recollection, identity, culture, literature, and architecture of Lviv today (Hrytsak and Susak, 2003; Henkel *et al.*, 2007; Boeckh, 2011, 2012).<sup>25</sup>

After World War II, the Socialist system all over Eastern Europe might even have reinforced conservative traits and institutions by fiercely striving to destroy them. The oppressive rules of the Communists who attempted to influence all aspects of public life provoked people to secretly cultivate a kind of civic culture. The fact that religious affiliations and nationalism survived the heavy socialist attacks suggests that also during the second half of the 20<sup>th</sup> century, hidden cultural norms motivated the behavior of people.<sup>26</sup>

From the theoretical and historical discussion, we infer two main hypotheses on the enduring effects of the Habsburg Empire that we want to test in this paper. First, because of the historical experience of a relatively decentralized, honest, and widely accepted state bureaucracy, we hypothesize that Habsburg positively affected people's trust in local public services. Second, we expect that this enduring effect on people's values reduces corruption in interactions with the local administration, despite the fact that formal institutions, laws, and legal regulations do not differ anymore between former Habsburg and non-

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<sup>24</sup> The fact that pluralistic judicial systems were maintained in a decentralized structure after 1918 is common in most Eastern European countries (Slapnicka, 1973; Küpper, 2005), as is the fact that civil servants from the Habsburg era remained in place (Sieghart, 1932).

<sup>25</sup> Furthermore, traits of social policy stemming from Habsburg traditions are clearly visible after World War I, as in the case of Czechoslovakia and its system to care about war veterans (Stegmann, 2014).

<sup>26</sup> After decades of Communism, church affiliation rates are still higher in Eastern Europe today than in Western Europe, according to data from the World Values Survey (Boeckh, 2013); nationalism has been argued to be one reason for the end of the socialist systems (Garton Ash, 1990).

Habsburg areas.

## 2. Data: Trust and Corruption in the Life in Transition Survey (LiTS)

Our main database is the Life in Transition Survey (LiTS) collected by the European Bank for Reconstruction and Development (EBRD) in 29 countries from August to October 2006.<sup>27</sup> The LiTS focused at surveying how the transition process after the fall of Communism affected people's lives. Besides socio-demographic information such as age, gender, and education, the survey collected information on trust in public services and whether respondents usually paid bribes in connection with these services.<sup>28</sup> In each country, the LiTS interviewed a representative sample of 1,000 households, consisting of 20 households in 50 locations (Primary Sampling Units, PSUs).<sup>29</sup> The LiTS dataset contains information on the municipality of residence of survey respondents. Figure 1 displays the location of municipalities in the LiTS dataset.

We restrict our analysis to countries that are either successor states of the Habsburg Empire or neighboring countries thereof. Austria, not being a transition country, is not part of the survey. Consequently, our dataset covers the following 17 countries in Eastern Europe: the former Habsburg countries Bosnia and Herzegovina, Croatia, Czech Republic, Hungary, Slovakia, and Slovenia; the non-Habsburg countries Albania, Belarus, Bulgaria, Former Yugoslav Republic of Macedonia, Moldova, and

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<sup>27</sup> Very recently, the EBRD made available a new wave of LiTS data, collected in late 2010. Because we aim to conduct the analysis as early after the fall of the Iron Curtain as possible and because the 2010 wave does not feature some relevant control variables like ethnic minority status, we restrict our analyses to the 2006 data here.

<sup>28</sup> Other studies that use the LiTS database to study trust and subjective preferences in the economic context include *Alghion et al.* (2010), *Grosjean* (2011), and *Grosjean and Senik* (2011).

<sup>29</sup> The PSUs are usually based on electoral registers or census enumeration areas. The total number of PSUs in the sampling frame per country ranged from 630 in Montenegro to 14,771 in Romania in our five-country sample. In the empirical analysis, we cluster standard errors by PSUs. Per household, one respondent was sampled following the Kish grid method (adult person in household who last had birthday).



Russia; and the partly-Habsburg countries Montenegro, Poland, Romania, Serbia, and Ukraine.<sup>30</sup>

For each municipality in our data, we collected information on affiliation with the Habsburg Empire, including its duration. Our sources for this information are Hrvatski Povijesni Atlas (2003), Kinder and Higemann (2004), Leisering (2004), Magocsi (2002), Reden (1995), Rothaug (2001), and additional specific investigations. Furthermore, we geo-coded municipality data to compute distances among the locations. We use this information below to restrict the sample to respondents in municipalities within a certain distance of the Habsburg border.

Our analyses focus on two main sets of outcome measures, reflecting trust and corruption in local public services. The LiTS questionnaire surveys trust in public institutions by asking, “To what extent do you trust the following institutions?” with a list of institutions including, among others, courts and police as well as what are mostly local public services and central state institutions such as political parties, the parliament, the government/cabinet of ministers, the presidency, and the armed forces, as well as trade unions as an example of a non-state institution. In contrast to other comparable surveys on trust (e.g., the World Values Survey or the US General Social Survey), the LiTS asks respondents to express the intensity of their trust beliefs.<sup>31</sup> The LiTS survey uses the following five-category scale: complete distrust, some distrust, neither trust nor distrust, some trust, and complete trust. We exclude the few observations reporting the additional category of “difficult to say” from our regression analyses (6.0 % for courts and 3.6 % for police, with no difference between Habsburg and non-Habsburg). In addition, the LiTS questionnaire also surveys trust in people by asking, “Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with people?”

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<sup>30</sup> Table 1 in the working-paper version (Becker *et al.*, 2011) documents the split of the 17-country sample, as well as the border sample, into Habsburg and non-Habsburg observations by country.

<sup>31</sup> The answer categories differ from those in the World Value Survey, which are either “Most people can be trusted” or “You can’t be too careful”. Given this formulation, the response may be “not only shaped by people’s beliefs about others’ trustworthiness, but also by their own preferences towards taking social risks” (Fehr, 2009, p. 239). The formulation in the LiTS with a scale from complete distrust to complete trust seems superior in this regard.



The LiTS questionnaire surveys corruption in public services by asking, “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” including courts and the road police as local public services.<sup>32</sup> The five-point categorical answer scale ranges from never over seldom, sometimes, and usually to always.<sup>33</sup> At a descriptive level, Appendix Table A.1 shows that distrust in and corruption of courts and police are higher in formerly non-Habsburg areas than in formerly Habsburg areas.

As a falsification test, we also analyze measures of membership in organizations, which is often used as a measure of social capital (Glaeser *et al.*, 2002). The LiTS questionnaire asks, “Are you a member of? (a) a political party, (b) other civic/voluntary organizations (club, association).” The respondent can answer with yes or no.

In addition to the outcome variables, the LiTS dataset provides a rich set of covariates observed at the individual level. These include respondents’ age, gender, education, religion, and indicators for whether respondents worked for income during last twelve months, for whether their native language is an official national language, and for whether they belong to an ethnic minority. In addition, household-level information is reported on household size, the number of children under age 14, and a set of possessions such as cars, credit cards, and mobile phones. Table 1 reports descriptive statistics of these variables for our 5-country sample (see Table 2 in Becker *et al.*, 2011 for descriptive statistics for the 17-country sample).

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<sup>32</sup> Another category refers to unofficial payments to “other police,” which is not further specified. Here, we restrict our analysis to the well-defined case of “road police.” However, results (available from the authors on request) are very similar for the remaining category, although mostly at lower levels of statistical significance.

<sup>33</sup> While it is hard to validate such corruption measures based on subjective responses, we can at least show external validity with respect to standard corruption measures at the country level: In our 17-country sample, the average of the LiTS corruption measures has a correlation coefficient with the Corruption Perceptions Index 2006 of Transparency International (2007) of .49, statistically significant at the 5 percent level.

### 3. Empirical Model

To estimate whether historical affiliation with the Habsburg Empire has a lasting effect on trust and corruption in local public services, our basic model expresses the outcome variables  $y$  – the measures of trust and corruption – of individual  $i$  living in location  $l$  in country  $c$  as a function of an indicator whether the individual lives in a community that historically used to be affiliated with the Habsburg Empire,  $H$ , and a set of individual-level and household-level control variables  $X$ :

$$y_{ilc} = \alpha + \beta H_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc} \quad (1)$$

Estimation at the individual level in a multivariate regression framework allows controlling for observable factors that vary systematically across individuals in our sample. We divide the rich set of control variables into two parts. The first set of controls includes variables that are arguably exogenous to the Habsburg effect – individual age and gender – as well as variables whose effect we want to separate from any possible effect of the historical Habsburg administration – language, ethnic minority status, and region. The second set of controls includes variables that may be viewed as endogenous to the Habsburg effect and are thus not included when estimating the reduced-form Habsburg effect – variables such as individual working status, sets of indicators of household property and of education, urban or metropolitan character of the community location, household size, and the number of young children. Given that the LiTS sampling frame drew 20 individuals per location  $l$ , we allow for clustering of the standard errors  $\varepsilon$  at the PSU level  $l$  throughout this paper.

#### 3.1 Border Specification

Identification in this setting is hampered by the fact that not all important factors may be readily observed. Most obviously, trust and corruption may differ across countries for reasons other than past

affiliation with the Habsburg Empire. When identification comes from cross-country variation between such former-Habsburg countries as the Czech Republic or Slovenia on the one hand and such non-Habsburg countries as Belarus or Moldova on the other, important general country characteristics such as aspects of formal institutions and geography may go unobserved. Such unobserved country heterogeneity, if correlated both with former Habsburg status and with cultural norms, would introduce omitted variable bias in the estimates of  $\beta$ .

To address such bias from omitted country variables, as a first step we include country fixed effects in the model specification. This model specification exploits the fact that the former Habsburg border cuts through several Eastern European countries. People living in these countries have been sharing a common set of formal institutions at least since the dissolution of the Habsburg Empire in 1918. But part of the population lives in territories that used to be part of the Habsburg Empire and another part in territories that did not belong to the Habsburg Empire. By including country fixed effects in the model, identification comes solely from variation within each of these countries. This effectively restricts identification to the sub-sample of countries that encompass both areas that used to be part of Habsburg and areas that were never part of Habsburg. This sub-sample includes five countries: Montenegro, Poland, Romania, Serbia, and Ukraine. The within-country share of individuals in our data who live on territory that used to belong to Habsburg is between 40 and 48 % in Montenegro, Poland, and Romania, while it is low in Ukraine and high in Serbia (see Table 1 in Becker *et al.*, 2011). Appendix A describes how, after 1918, these countries ended up combining Habsburg and non-Habsburg parts. It points out that the newly formed countries mostly united historically related territories and that the Habsburg border did not coincide with ethnic separations.

While within-country identification solves the most obvious issues of unobserved heterogeneity, countries like Poland, Romania, and in particular Ukraine still have such a sizeable geographical

expansion that important factors may yet remain omitted from the model. Therefore, as a second step, we further restrict the sample to individuals  $i$  living within a restricted band around the former Habsburg border:

$$y_{ilc} = \alpha_c + \beta H_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc} \quad \text{if } i \in \text{border region} \quad (2)$$

where  $\alpha_c$  is a full set of country fixed effects and border region refers to a close geographical band around the Habsburg border. The Habsburg border – no longer existent at least since 1918 – gives rise to a geographic discontinuity within the five-country sample. This border specification identifies the Habsburg effect by comparing individuals left and right of the former Habsburg border living in reasonable proximity to one another. Proximity of residence, within a common region that is divided by a non-existing border, is meant to induce similarity in important unobserved variables. To balance the desire for a narrow band to ensure similarity against the desire for large enough samples to retain statistical power, our basic model compares individuals living in communities located within 200 kilometers (124 miles) of each other on either side of the historical Habsburg border. In robustness analyses, we also report results for even narrower bands, going down to bandwidth as small as 25 km (15 miles).

To ensure that the “control group” to which the Habsburg “treatment group” is compared does not include locations that had actually also been exposed to the Habsburg treatment at some time, throughout the definition of the Habsburg variable  $H$  is an indicator of whether a location has ever been part of the Habsburg Empire. By contrast, defining the Habsburg variable by Habsburg affiliation at any particular point in time would mean that part of the “control group” had also received a “Habsburg treatment” at some point in history. For example, defining the Habsburg treatment by its borders just before its demise in 1918 would mean that several regions that had been part of Habsburg until 1908 would constitute a substantial part of the control group in the border sample. However, as one robustness analysis below, we

show that results are robust in a specification that restricts the analysis to locations that were part of the Habsburg Empire in 1900 (and their control locations) while dropping all locations from the analysis that had been part of the Habsburg Empire at some time but were no longer in 1900. While the specification of our basic model assumes that the Habsburg treatment effect is independent of the duration of treatment, in additional specifications we also test whether the Habsburg effect differs by length of affiliation with the Habsburg Empire.

To ensure even better comparability of the treatment and control groups on the two sides of the former Habsburg border, rather than literally including any observation within 200 km from the Habsburg border in the sample, we implement the border sample using a geographic matching algorithm. Individuals are included in the border sample only if there is an observation in the dataset within 200 km on the other side of the Habsburg border. That is, observations enter the sample only if reasonably close-by observations exist on the other side of the border. This means that the bandwidth is effectively much smaller than 200 km, because there is usually no community placed directly at the border. It also means that the effective bandwidth varies across regions depending on whether a match exists on the other side of the border or not. This way, our results are not driven by observations that cannot be matched with similar close-by observations. The border sample is illustrated in Figure 1, where the black dots depict the locations of the LiTS observations in the band of 200 km around the former Habsburg border.<sup>34</sup>

Columns (5)-(8) of Table 1 compare the Habsburg and the non-Habsburg part of the border sample in terms of their background variables. The fact that there are hardly any differences confirms that the border specification focuses the analysis on a treatment and control group that are very similar apart from their difference in Habsburg treatment status. The only differences that seem to come out are a higher share of

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<sup>34</sup> Note that given the definition of the Habsburg variable by whether the individual lives in a location that has *ever* been part of the Habsburg Empire, the relevant Habsburg “border” actually never existed in the specific shape, but is defined by the enveloping shape of Habsburg borders at any time in history.

respondents from an ethnic minority on the Habsburg side and the somewhat higher level of education on the Habsburg side. The former is consistent with the inclusive nature of the Habsburg rule, the latter with their stronger education efforts. Note also that the Muslim share is in fact higher on the *Habsburg* side. In our regressions, we control for these and other variables to account for these limited differences on the two sides of the border.

Our empirical identification is designed to identify a specific mechanism through which the history of the Habsburg Empire affects outcomes today. Of the four main mechanisms discussed in the literature (see Section 1.1 above) – formal institutions, geography, human capital, and cultural norms – we aim to shut down the first three ones. By identifying within regions that have been part of one country for more than 90 years,<sup>35</sup> we exclude the possibility that Habsburg history matters through differences in formal institutions like written laws today. By focusing on a narrow band of observations, we also exclude the channel that history matters because of geographical differences (which we further test below). Finally, having been part of the same country for three generations since 1918 means that formal education systems have been the same, and we control for differences in educational attainment at the individual level. This setup allows us to focus on the effect of historical affiliation with the Habsburg Empire on observed cultural norms today, including aspects of the latter that may be intertwined with the functioning of formal institutions or with educational content.

In Section 5 below, we test the validity of the border identification model in several falsification tests. Amongst others, we spuriously move the border by 100 km to either side, to ensure that the estimated effect does not simply capture linear effects of the outward expansion of the Habsburg Empire. We test for geographical differences of observations on either side of the border, to ensure that the Habsburg border does not coincide with obvious geographical barriers. We also test whether observations on either

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<sup>35</sup> While this is true for all five countries, not all of them existed in their current borders for the past 90 years. The Ukraine belonged to the USSR and Serbia and Montenegro to (the Kingdom and later Republic of) Yugoslavia.

side of the border differ in dimensions that existed before the expansion of the Habsburg Empire, such as medieval city sizes and the location of medieval trade routes and diocesan cities. Finally, we contrast the results on trust in public services with interpersonal trust. All specification tests corroborate the validity of the border specification with respect to trust and corruption in local public services.

The measures of trust and corruption are categorical variables indicating the strength and degree of ubiquity of these conditions. To account for the ordered nature of the outcome variables, we estimate ordered logit models, which have the following form:

$$\begin{aligned}
 \text{logit}(p_1) &= \log(p_1)/(1 - p_1) = \alpha_1 + \beta' X \\
 \text{logit}(p_1 + p_2) &= \log(p_1 + p_2)/(1 - p_1 - p_2) = \alpha_2 + \beta' X \\
 &\vdots \\
 \text{logit}(p_1 + p_2 + \dots + p_k) &= \log(p_1 + p_2 + \dots + p_k)/(1 - p_1 - p_2 - \dots - p_k) = \alpha_k + \beta' X \\
 &\text{with } p_1 + p_2 + \dots + p_k + p_{k+1} = 1
 \end{aligned} \tag{3}$$

where  $X$  here contains both the indicator for affiliation with the Habsburg Empire and the set of control variables. In this proportional-odds model, the odds ratio of the event is independent of the category  $j$ . The odds ratio is assumed to be constant for all categories. Alternatively, we estimated a generalized ordered logit model which allows for non-proportional odds (Williams, 2006; see also Maddala, 1983; McGilchrist, 1997). Results are very similar. Another alternative is to ignore the categorical nature of the outcome variable and perform ordinary least squares (OLS) regressions (e.g., Blanchflower and Oswald, 2004). Again, results are qualitatively similar.

#### 4. Geographic Regression Discontinuity Design

Our border identification strategy has some similarity with a geographic regression discontinuity design (RDD) in that it exploits a discontinuous geographic variation (Lee and Lemieux, 2010), but it also has important differences. First, our historical setting of a long-gone political border contrasts with modern-day examples of a geographic RDD where the immediate (short-run) impact of the introduction

of some policy in one geographic area is contrasted with a control on the other side of a border. For example, rather than being orthogonal to other observable factors, we do actually *expect* the Habsburg treatment to have affected other covariates in the model, as it has been affecting trust and corruption in local public services for much more than a century now. What is relevant is that the Habsburg treatment can historically be viewed as “exogenous”, and we will test this in a battery of falsification tests below.<sup>36</sup> Second, the LiTS sampling framework drew 50 municipalities per country across the whole country and did not zoom in on the Habsburg border. As a consequence, there are relatively few observations located directly at the border, making the identification of sharp jumps at the discontinuity difficult. Third, in sparsely inhabited areas the exact drawing of the historical Habsburg border is unclear and disputed even among historians (e.g., Ingrao *et al.*, 2011). The matching algorithm of our border specification only requires the coding of the Habsburg status of each specific location but not the drawing of the border, whereas calculating distance to the border in the RDD approach does require an exact drawing of the border.

Fourth, and most importantly, in contrast to the basic assumption of an RDD, there are a number of reasons to expect some sort of diffusion to and interdependence between neighboring towns across the former Habsburg border. Such reasons include migration and marriage between neighboring towns; local spillovers whereby inhabitants of an Ottoman town just across the border observe that well-functioning public services may have positive consequences, so that they may imitate them to some extent; “frontier” effects in that authorities may behave differently if located close to the enemy, such as when a Habsburg local authority may behave more authoritarian if the Ottomans are next door; and political competition across neighboring locations. All of these effects would mean that the Habsburg status of one town may affect outcomes in towns that are located directly on the other side of the border. Note that any of such

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<sup>36</sup> Ideally, to estimate the historical Habsburg effect one would like to have historical information on changes in trust of individuals who came under Habsburg rule and of their contemporaries who did not. Unfortunately, no such data exist.



diffusion or interaction effects work against our identification strategy finding a significant difference between Habsburg and non-Habsburg locations in the border sample.

Despite these differences from a classical RDD setup, for comparison and to ensure that our results are not driven by peculiarities of our matching algorithm, we also perform standard RDD models that control for polynomials of geographic location:

$$y_{ilc} = \alpha_c + \beta H_{ilc} + X'_{ilc} \gamma + f(\text{geographic location}_i) + \varepsilon_{ilc} \quad \text{if } i \in \text{border region} \quad (4)$$

where  $f(\text{geographic location}_i)$  is a polynomial that controls for smooth functions of geographic location.<sup>37</sup>

A natural starting point is to control for polynomials in distance to border (e.g., Black, 1999; Lalive, 2008). Dell (2010) proposes to go beyond such a one-dimensional geographic RDD and to use a two-dimensional RDD in latitude-longitude space that employs polynomials in latitude and longitude. We show results for both approaches and for different degrees of polynomials, as well as for different sizes of bandwidth around the border (from 25 km to 200 km). In the RDD specifications, these samples are defined in terms of GIS-computed distance to the former Habsburg border, rather than by the matching algorithm used in the border specification.

In a setting with sharp geographic discontinuities in treatment, the RDD approach will provide cleaner identification of short-run immediate effects. At the same time, in our setting of effects of long-gone borders, it may be prone to attenuation from diffusion and interaction effects across the border. Given the pros and cons for both the border specification and the geographic RDD specification, we report results for both types of specification throughout the paper.

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<sup>37</sup> A further generalization replaces the country dummies, which are essentially (longer) border segments, by indicators for (shorter) border segments. Our results are robust to this generalization (not shown).

## 4. Results

This section reports our main results on the effect of historical affiliation with the Habsburg Empire on current people's trust in two essential public services with which citizens interact –courts and police – and on real consequences for corruption in the citizen-state interactions.

As a starting point, Table 2 uses the full sample of 17 countries that were formerly part of the Habsburg Empire or neighboring countries thereof. All models control, at the individual level, for age, gender, native language, ethnic minority status, and six categories of religious affiliation. The results show that living in a location that used to be part of the Habsburg Empire is significantly positively associated with higher levels of trust in courts and trust in the police. These higher levels of trust also show up in the actual interactions between citizens and the two types of public authorities, in terms of significantly lower incidences of unofficial payments when interacting with the courts and with the road police. To account for the fact that such assessments may depend on whether respondents had contact with the specific public service at all, the latter specifications control for whether respondents used the service in the previous twelve months. Respondents with contacts report significantly higher levels of corruption.

### *4.1 Border Specification*

Because results in the 17-country sample might be partly driven by cross-country differences in trust and corruption, our border specification restricts the analysis to more readily comparable comparison groups. In Table 3, we use the border sample of respondents living within 200 km of the former Habsburg border in those five present-day countries that were partly Habsburg. This reduced sample contains about 3,600 observations, but the clustering in 180 locations means a substantial reduction in statistical power. The models include country fixed effects, so identification comes from within-country variation close to

the former Habsburg border. The results show that living in a former Habsburg location has a significant positive effect on the reported level of trust in courts, with a point estimate somewhat larger than in the 17-country sample. The point estimates on the other three variables are smaller than in the 17-country sample, indicating that those results may be biased upwards by unobserved cross-country differences. Still, also in this specification, Habsburg affiliation has a significant negative effect on the corruption of courts and road police, and the positive effect on trust in police reaches statistical significance at the 12 percent level. Thus, while statistical precision is somewhat lower in the more demanding border specification, the results indicate that the Habsburg Empire had a significant effect on higher trust and lower corruption in local public services.

Going beyond statistical significance, we can discuss the size of the effects estimated by the ordered logit models by referring to marginal effects. All dependent variables are measured in five categories, with answers on the trust variables ranging from “complete distrust” to “complete trust” and answers on the bribe variables ranging from “never” to “always.” As the detailed results reported in Appendix Table A show, when holding the other variables constant at their means, having been part of the Habsburg Empire increases the probability of moving to a higher category of trust in courts by 2.3 percentage point, on average across the five categories. The average of the absolute value of the percentage change across categories is 1.6 percentage points for trust in the police, 2.4 percentage points for bribes to courts, and 2.9 percentage points for bribes to the road police. Viewed relative to the average share of 20 % in each of the five categories, Habsburg affiliation thus moves the trust and corruption categories by 8-15 % on average. This is substantially larger than the average absolute marginal effects of variables such as gender and age, and in the same order as speaking the native language or belonging to an ethnic minority, albeit smaller than the average absolute marginal effects of some indicators for religious groups. Trust and corruption in courts and police are thus affected by former Habsburg affiliation in a quantitatively

important way.

Appendix Table A.3 shows the marginal effects of Habsburg affiliation category by category. For instance, affiliation with the Habsburg Empire reduces the probability of reporting complete distrust in courts by 4.8 percentage points and increases the probability of reporting some trust by 3.4 percentage points. The categories of complete distrust on the one hand and some trust and complete trust on the other hand are also the most affected ones in the case of trust in the police. By contrast, the main effect of Habsburg on corruption of courts and road police appears in raising the category of never having to bribe by 5.9 and 7.2 percentage points, respectively, with most of the reduced incidence appearing in the categories of seldom and sometimes.

#### *4.2 Geographic Regression Discontinuity Design*

Table 4 reports results of different specifications of geographic RDD models. The top panel refers to the two-dimensional geographic RDD suggested by Dell (2010). The reference specification, reported in the first row, controls for a quadratic polynomial in latitude and longitude. Results are very similar to the border specification. In fact, point estimates are somewhat larger in the RDD model and reach statistical significance at conventional levels for all four outcome variables.

The next three rows replace the quadratic polynomial in latitude and longitude with linear, cubic, and quartic polynomials, respectively. In these specifications, the estimates in the trust models do not reach statistical significance at conventional levels. By contrast, the results on bribes to courts are highly robust even with the very demanding higher polynomials.

The bottom panel of Table 4 reports results of more traditional one-dimensional geographic RDD models that control for linear and quadratic polynomials in distance to border, respectively. The bottom row also reports a model that interacts the distance-to-border term with the Habsburg dummy. Again, results are highly robust, with statistical significance larger in the bribes models than in the trust models

(although all reported coefficients in the trust models reach statistical significance at the 10.3 to 13.7 percent level).

#### *4.3 Robustness Checks*

In any identification from geographical discontinuities, choice of the width of the band around the border is subject to a trade-off between geographical proximity and sample size. Table 5 thus reports results both for the border specification and for the RDD reference specification for samples restricted to land within 200 km, 150 km, 100 km, 50 km, and 25 km of the border, respectively. While the trust results tend to lose statistical significance in the smaller samples, in particular the results on bribes to courts prove statistically significant in samples even as close as 25 km (15 miles) on either side of the former Habsburg border. While reduced statistical precision is not surprising in the more demanding specifications – in the 25 km band, the number of PSUs is as small as 62 across the five countries – it is reassuring that the point estimates prove quite stable in the specifications with smaller bandwidths.

While the control variables included so far are arguably exogenous to the Habsburg treatment, additional control variables available at the individual and household level are potentially affected by past Habsburg affiliation. In principle, affiliation with the Habsburg Empire might have affected levels of income and wealth as well as education, urbanization, and fertility, so these variables might constitute channels through which the Habsburg effect comes about. However, as shown in Table 6, although the additional control variables increase the variance in trust and corruption accounted for by the models, results on the effect of having been part of the Habsburg Empire are hardly affected. This suggests that property, education, urbanization, and fertility are unlikely to be major channels of the Habsburg effect. For the remainder of the paper, we stick with this longer set of control variables.

Several additional tests confirm the robustness of our main results (details available on request). To check whether results depend on any particular country in the border sample, we run five separate regressions where

we drop one country at a time. The reported results are robust in this analysis. In addition, OLS regressions ignoring the ordered nature of the trust and corruption outcomes also yield the same qualitative results.

## 5. Falsification Tests

This section provides deeper analyses of whether the results so far can be interpreted as a genuine effect of the Habsburg Empire on trust and corruption in local public services. They are devised to test whether we can interpret our border sample analysis as a “historical experiment” where Habsburg affiliation can be viewed as “randomly assigned.” They aim to rule out that the identified effect reflects other things such as selective expansion of the Habsburg Empire, in which case the results may reflect pre-existing differences. Appendix A provides narrative background that as a general pattern, in our border sample the Habsburg border separated quite homogeneous areas, so that the Habsburgs did not just take over those areas where people were already more trusting. To test this empirically, this section provides a number of falsification tests, several of which are derived from regression discontinuity analysis (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). We check whether there are treatment effects when spuriously moving the Habsburg border inwards or outwards; whether the Habsburg border is associated with differences in geographical characteristics or indicators pre-dating the Habsburg era; and whether Habsburg effects emerge in interpersonal interactions.

### 5.1 *Pseudo Borders: Moving the “Treatment” Inward or Outward*

A potential concern with the findings so far is that, rather than capturing a genuine Habsburg effect, they might only reflect continuous differences in the West-East dimension or in remoteness from Austria. The first falsification test is therefore to check whether a similar “effect” is found when spuriously moving the border inward or outward. There should not be an effect when comparing locations on either side of

these meaningless borders.

To keep comparability to the previous analyses, we stick with the five-country border sample and implement the falsification test as follows. In a first exercise, estimation is restricted to the Habsburg municipalities in the 200 km band of the true Habsburg border and “treatment” is defined as being “to the left” of a pseudo border that is moved 100 km inwards relative to the true Habsburg border. In the second exercise, estimation is restricted to the non-Habsburg municipalities in the 200 km band of the true Habsburg border and “treatment” is defined as being “to the left” of a pseudo border that is moved 100 km outwards relative to the true Habsburg border. This exercise effectively restricts the analysis to two 100 km bands around borders that are spuriously moved inwards and outwards by 100 km and is thus directly comparable to the results of the 100 km border sample just discussed.<sup>38</sup>

Results show no statistically significant “effect” of these pseudo borders whatsoever (Appendix Table A.10). In fact, the majority of estimates points in the “wrong” direction, with lower trust and higher corruption on the apparent “Habsburg” side of the analysis. These findings support the interpretation that our results capture a genuine Habsburg effect and not just a West-East difference.

### *Geographical Comparison of Habsburg and Non-Habsburg Areas*

A second potential concern with the analysis so far is that during its expansion, the Habsburg dynasty might have chosen to expand only into certain types of geographical regions and, for example, stop at more mountainous regions. We therefore analyze possible jumps in the value of this geographical measure at the Habsburg border. We restrict this robustness check to this geographic characteristic because individual and household characteristics might potentially vary as a result of the Habsburg Empire, whereas this geographic feature is arguably exogenous.

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<sup>38</sup> Note that moving the whole band by 200 km would move the pseudo border outside our five-country sample.

Thus, we regress the altitude of the municipalities in the border sample on a Habsburg indicator (see column (1) of Appendix Table A.5). The coefficient on the Habsburg indicator is 37.2 meters ( $t$ -value 0.91) in the border specification and 33.1 meters (0.60) in the RDD specification. Habsburg municipalities thus do not systematically differ in altitude compared to non-Habsburg municipalities. This finding dilutes potential concerns that the Habsburgs might have fought their way through some valley and were stopped at a mountain, or, conversely, that they systematically chose mountainous locations in proximity to their neighbors – either of which might potentially be correlated with attitudes towards trust and corruption.

### *5.2 Comparison of Pre-Existing Factors between Habsburg and Non-Habsburg Areas*

Another potential concern is that the results found after the fall of the Habsburg Empire only perpetuate pre-existing differences before the Habsburg Empire came into being (see also Appendix A for alternative background that dilutes this concern). In principle, the Habsburg Empire might have expanded into areas that were distinct from areas outside the (new) Habsburg border in dimensions related to our outcomes. For instance, the expansion of the Habsburg Empire might have stopped short of areas that were less economically developed and might have been harder to develop. Similarly, areas outside the Habsburg Empire might have differed in their values, beliefs, and levels of trust already before the Habsburg Empire came into being.

To address these issues, we collected a series of variables capturing economic development, exposure to outsiders, and cultural features that pre-date the expansion of the Habsburg Empire. We then test whether there are significant differences in the variables between municipalities on both sides of the Habsburg border. In contrast to the previous robustness check that uses a time-constant geographic characteristic, this check uses indicators that might vary over time.

Economic historians often use urban population as a proxy for pre-industrial economic prosperity because cities could only be supported in areas with high agricultural productivity, advanced economic



specialization, and developed transport systems (Bairoch, 1988; Acemoglu *et al.*, 2002). We use data on urban population from Bairoch *et al.* (1988) to construct a measure of urban population in cities in our sample of Eastern European countries. We use city size in 1400, well before the Eastern expansion of the Habsburg Empire, as an indicator of economic development. Cities inside and outside the Habsburg borders do not differ systematically in population size (see column (2) of Appendix Table A.5).

Our second measure uses major trade routes in 1450 as indicators of interaction with foreign traders. Exchange with foreign parties is likely to affect the trust levels of people. When regressing an indicator of major trading cities, derived from Magocsi (2002), on our Habsburg indicator, we do not find a statistically significant effect (column (3)).

Finally, cultural values in different parts of Eastern Europe might have been influenced by a strong presence of the Church. We use an indicator variable for whether a city was a diocesan town in 1450, also from Magocsi (2002). Again, the location of diocesan towns does not vary significantly by later affiliation with the Habsburg Empire (column (4)).

This last set of robustness checks, although somewhat limited by data availability on proxies for pre-existing cultural differences, suggests that the Habsburg Empire did not systematically expand into certain areas to exploit pre-existing advantages in terms of economics, trust, and values. Together, the three sets of robustness checks suggest that the provided analyses identify a genuine effect of the former Habsburg border and do not pick up confounding effects.

#### *5.4 Habsburg Affiliation and Interpersonal Trust and Interactions*

Our main hypothesis is that the localized and well-respected Habsburg administration had long-term effects on trust and corruption in local public services. The historical background suggests that the Habsburg Empire affected person-state interactions but interfered little with local cultures and interpersonal interactions (Section 1.2). A further falsification test is therefore whether historical

Habsburg affiliation affects interpersonal trust and social capital.

In the same way that LiTS surveyed trust in courts and police, it also surveyed trust in other people and private entities. As shown in column (1) of Table 7, there is no significant effect of Habsburg affiliation on trust in other people, and the point estimate is actually negative.<sup>39</sup> Similarly, column (2) shows that there is no significant Habsburg effect on trust in trade unions, a prominent example of a private entity with which many people interact that is surveyed by the LiTS. The same is true for trust in non-governmental organizations (not shown).<sup>40</sup> The fact that these two institutions hardly existed or played only a marginal role during the Habsburg Empire underscores the falsification character of this exercise.

Another measure of interpersonal interaction is the extent of membership in organizations, which is commonly used as a measure of social capital (see Glaeser *et al.*, 2002). LiTS respondents are asked separately about membership in political parties and in civic organizations. Habsburg affiliation does not have a significant effect on either of these measures (columns (3) and (4)).

Overall, the Habsburg Empire does not seem to exert persistent effects on trust in people and private entities and on social capital and the engagement of citizens in their community. This corroborates the result that the Habsburg effect is one related to person-state interactions at the local level and not to any kind of interaction between individuals or between individuals and private entities, which has no relation to past and present experiences of the local bureaucracy. These results also provide additional evidence that the Habsburg expansion is unlikely to have been selective, as it affects only a specific aspect of trust and not trust in general.

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<sup>39</sup> The LiTS questionnaire surveys trust in other people not only for “today”, but also for “before 1989” (note that only this particular question looks into pre/post 1989); Habsburg affiliation is similarly insignificant in predicting the latter variable.

<sup>40</sup> Similarly, Habsburg affiliation does not have a significant relation (and actually a positive point estimate) with having to pay bribes for medical treatment, another area not linked to bureaucratic legacy.

## 6. Complementary Evidence from a Business Survey

Results so far refer to the experience of *individuals* with local public services. In terms of economic relevance, the trustworthiness and impartiality of local public services in *business* disputes may have specific relevance. In particular when it comes to trust in the court system and to sizable bribes, the experience of firms may be of particular interest. As a final addition, we therefore complement the evidence on trust of individuals in local public services by looking at the experience that firms have.

Suitable data come from the Business Environment and Enterprise Performance Survey (BEEPS), which was conducted by the EBRD and the World Bank in Eastern European countries in 2005. The purpose of the survey was to better understand constraints that hinder the development of businesses. Businesses were asked to answer in a fashion reflecting “only [their] perception and experience of doing business in [their] country.” The BEEPS sampling aimed at representativeness in manufacturing and services. Similar to the LiTS, also in the BEEPS we know the location of respondents. From this, we geocoded municipality data to compute distances and again restrict the sample to respondents in municipalities within 200 km of each other on either side of the former Habsburg border in the five-country sample. This yields a border sample of roughly 1,000 firms.

In surveying firms’ business experience, the BEEPS asks questions about the role of the court system in resolving business disputes. Additional information allows us to control for eight sector dummies, the year when the firm began operations in the country, and indicators for firm size and ownership structure, as well as country dummies. As shown in Table 8, firms on the Habsburg side of the former border are significantly more likely to consider courts as being fair and impartial. There are also positive point estimates, albeit mostly short of statistical significance at conventional levels, on considering courts as being honest and uncorrupted and on firms’ confidence that the legal system will uphold their contract and property rights in business disputes. Possibly, the wording of the latter question (“legal system”) may

draw firms to think also of the functioning of the central legal system as opposed to the local courts.

Comparable questions on trust and corruption of the police are not available in the business survey.

The BEEPS also surveys membership in business associations or chambers of commerce. We do not find a significant Habsburg effect on this, with the point estimate even negative (not shown). Again, the Habsburg effect is not visible in inter-firm interactions but only in firm-state interactions in terms of firms' perception of the courts being fairer and more impartial.

Results based on the business survey thus confirm the household survey results: Firms on the Habsburg side of the long-gone border within the same country have higher trust in the courts.

## **7. Conclusions**

Our results show that even nearly a century after its demise, the Habsburg Empire persists in the attitudes towards and interactions with local state institutions of the people living within its former borders. Comparing individuals living on either side of the long-gone Habsburg border, we find that respondents in a current household survey who live on former Habsburg territory have higher levels of trust in courts and police. They are also less likely to pay bribes for these local public services, demonstrating that the institutional heritage influences not only preferences and unilateral decisions but also bilateral bargaining situations in citizen-state interactions. In fact, the most robust finding is a reduced propensity to bribe the courts, whereas results on bribes to the road police and on trust are less precisely estimated in more demanding model specifications.

We establish this result on the basis of two empirical strategies – a border specification and a two-dimensional geographic RDD – that are identified from within-country geographic variation. We exploit the fact that the historical Habsburg border cuts through five modern-day countries in Eastern Europe. Using this five-country sample, we identify the Habsburg effect by comparing communities in close

geographic proximity within a range of 200 km on either side of the former Habsburg border. This approach has the advantage that respondents have been sharing a common set of formal institutions for a long time now because they live within the same current national borders, but differ in their historical exposure to the formal institutions of the Habsburg Empire. We also control for observed variations in education, religion, language, and wealth at the individual level. Given this setting, we rule out most aspects of other channels of historical influence discussed in the literature, such as geography, formal institutions in the form of written laws, and human capital in the form of educational degrees. In the case of the Habsburg Empire, history matters for current attitudes and behavior not because formal institutions persisted, but because individual cultural norms with regard to local public services survived. A series of falsification tests corroborate this interpretation.

Our results have several additional suggestive aspects (see Appendix B for details). They are robust when restricting the comparison groups to formerly Ottoman regions. But the analyses cannot distinguish differential effects for alternative neighboring empires. Moreover, there is no strong evidence of Habsburg effects on trust in central public services.

Results on effects of the duration of exposure to the Habsburg Empire and on the specific mechanisms through which the Habsburg effect has survived are also necessarily speculative. The limited within country variation in duration of Habsburg affiliation exploited in our border analysis prevents strong conclusions, but at least does not provide supportive evidence that the effect varies with the duration of exposure (see Appendix B.2), suggesting that even transitory interventions can have long-persistent effects. Since the demise of the Habsburg Empire, substantial waves of migration and displacement accompanied the institutional disruptions that the successor states of the Habsburg Empire have experienced. This suggests that the cultural norms of behavior are unlikely to have survived solely through intergenerational transmission within families, but rather also through channels such as the persistent

nature of continuous reciprocal interactions in local communities, the content of knowledge and behavioral patterns conveyed in schools, or the quality of human capital of citizens, judges, and policemen. Additional analyses that investigate in greater detail the mechanisms through which the empire effect comes about and prevails – presumably requiring different areas of application – constitute interesting directions for future research.

At a general level, our application of the Habsburg Empire shows that past formal institutions can leave a legacy through cultural norms even after generations of common statehood. For the understanding of the functioning of state institutions in general, this implies that good governance can generate a cultural basis that facilitates collective action long into the future.

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## **Appendix A: Post-1918 Emergence of Countries with Habsburg and Non-Habsburg Parts**

Our identification in the border sample notably derives from the fact that there are five modern-day countries that combine territories that were and were not part of the Habsburg Empire. It is important to analyze how this formation came about so that countries ended up combining the different parts. In particular, understanding the background of this helps ease concerns that the within-country separation by the former Habsburg border might be correlated with ethnic or cultural differences that date back even further in history.

After World War I, the peoples, in particular within the defeated Habsburg Monarchy, pressed for the

right of self-determination. With that premise, many borders were newly defined to merge peoples with a common language. Consequently, as a general rule (which by necessity hides specific historical exceptions), the combination of Habsburg and non-Habsburg parts in one country meant that areas that had been reasonably natural units before were now joined together (again). In Romania and the Ukraine, areas that already previously tended to be inhabited by ethnologically common people with a common language joined. The same is true for the Galician parts of Poland. In Serbia, a national assembly of the former Habsburg part deliberately voted to join the non-Habsburg part after World War I. In Montenegro, the former Habsburg affiliated and non-Habsburg parts formed a natural geographical unit for a long time. The one clear exception to this general pattern is the Silesian part of Poland, where a different area was integrated into an otherwise quite foreign country.<sup>41</sup>

The relative internal homogeneity of the forming countries even before they came into being after World War I also documents itself in the fact that their political borders were already visible in the economy well before the new countries were formed, as shown by Schulze and Wolf (2009, 2012). Their studies document, furthermore, that the fact that the fault lines along which the Habsburg Empire was to break up are evident long before its demise cannot be explained by factors such as administrative barriers, geography, or infrastructural differences.

The following provides a rough overview of the cases of the five countries which, by focusing on the general patterns, of course does not do justice to all specifics.

*Poland:* Polish population had been living in the Polish parts of Galicia before they came to Habsburg. Galicia's main part joined the Habsburg Empire in the First Partition of Poland in 1772 and stayed until 1918. North Galicia joined the Habsburg Empire in the Third Partition of Poland in 1795 and stayed only until 1809. That is, Galicia used to belong to Poland, then came to Habsburg through power

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<sup>41</sup> In line with this exception status, the analyses in Appendix B.2 reveal that results get stronger when disregarding Silesia.

politics driven by Russia and Prussia, and then returned to Poland after World War I. The situation is quite different in Silesia. Silesia used to be under the rule of the Crown of Bohemia, which passed to Habsburg in 1526. After the Silesian Wars, it was incorporated by Prussia in 1742 and later remained part of the Weimar Republic. Only after World War II did it join Poland.

*Ukraine:* East Galicia and northern Bukovina used to be part of Habsburg, with strong Ukrainian populations. In connection with World War II, they became part of Soviet Ukraine.

*Romania:* Three historical principalities form Romania: Wallachia, Moldova, and Transylvania. They all had large Romanian population. Whereas Transylvania used to be close to the Habsburgs and part of Hungary, the other two were ruled indirectly by the Ottomans. Interestingly, already in 1600 the three were briefly united. In 1859, Moldova and Wallachia formed the modern Romanian state. Transylvania was passed to it after World War I as an area of Austria-Hungary with a Romanian population.

*Serbia:* The Vojvodina, which had been part of Habsburg resp. Hungary, came to the Kingdom of the Serbs, Croats, and Slovenes after World War I. A quickly constituted national assembly actually voted in favor of joining the Serbian kingdom in 1918. After World War II, it remained an autonomous province in the Yugoslav Republic of Serbia. Other parts of Serbia that had been affiliated with Habsburg are the so-called “Kingdom of Serbia” south of the rivers Sava and Danube, ruled by the Habsburg crown from 1718-1739 and 1788-1791, and the Serbian part of the Sanjak, where Habsburg garrisons were stationed from 1878-1908.

*Montenegro:* The gulf and harbor of Kotor had offered access to the Adriatic Sea to the Serbian empire in the Middle Ages and later to the Republic of Venice, before it became part of the Habsburg crown land Dalmatia in 1797 (with the short Napoleonic interference of 1805-1815). Since 1918, the Bay of Kotor belonged to the Kingdom of the Serbs, Croats, and Slovenes (since 1929: Kingdom of Yugoslavia), first within the Zeta Banovina (an administrative body), after World War II as part of the

Yugoslav Socialist Republic Montenegro. Another area formerly affiliated with Habsburg by garrisons is the Montenegrin part of the Sanjak (1878-1908).

## **Appendix B: Additional Aspects of the Habsburg Effect**

This appendix probes particular aspects of the Habsburg effect in greater depth. In particular, we analyze whether the effect of the Habsburg Empire differs by alternative neighboring empires, whether the duration and timing of Habsburg affiliation alters the effect, and whether Habsburg affiliation has an effect on trust in central public services.

### *B.1 Alternative Neighboring Empires*

In the analyses so far, affiliation with the Habsburg Empire has been defined as a binary variable. The implicit assumption is that affiliation with the Habsburg Empire has a homogeneous effect, independent of the comparison group on the other side of the border and independent of the duration of affiliation. Here we investigate whether there is in fact heterogeneity in these dimensions.

*Historical background.* Three different empires ruled in the area that forms the control group for the Habsburg treatment: the Ottoman Empire, Russia, and Prussia. The Habsburg political structure and administration described in Section 1.2 above differed in important aspects from these Eastern neighbors (for comparative treatments, cf. Ingraio, 1996; Subtelny, 2007).

The Ottoman Empire pursued no comparable reforms (see Jones, 1987, ch. 9, for an overview). It has been characterized as a “typical despotism” (Landes, 1998, p. 398), a large entity with exploiting rulers, politically and economically stagnant since the 17<sup>th</sup> century. Its subjects (*raya*, “protected flock” of the sultan) were deprived of political influence, and reforms in the late 19<sup>th</sup> century remained relatively weak. Hardly any secular education existed. Bribery was an accepted institutionalized phenomenon, perceived

as normal, even expected by officials (Shaw, 1976; Imber, 1990; İnalcık, 1996; Eldem, 2006). Corruption constituted a systemic trait of the entire administrative system and had become an intrinsic and hardly avoidable part of local administration that alienated the subject peoples (Findley, 1989).

Russia conceded some economic and social modifications – serfdom was abolished in 1861, compared to 1781 in Habsburg. But the autocratic monarchy gave no leeway to real parliamentary influence until the end of the monarchy in 1917 and did not allow for decentralized political development (Bartlett, 2005; Subtelny, 2007). Prussia, by contrast, while also known for a well-developed bureaucracy and education system, conceded less autonomy to its outer territories than Habsburg.

*Results.* In our border sample, nearly three quarters of the observations in the three control empires belonged to the Ottoman Empire, with Russia and Prussia dividing the remaining observations. Consequently, while individual sample sizes for the Russian and Prussian alternatives are too small to yield enough statistical power, one robustness specification is to restrict the control group to only those areas that belonged to the Ottoman Empire. Our results, reported in the upper panel of Table A.6, are robust in this restricted sample (with the trust results shy of statistical significance in the RDD specification).

If, alternatively, we use the full border sample and include a dummy for those non-Habsburg areas that were *not* part of the Ottoman Empire, the dummy is never significant. This indicates that we cannot detect a statistically significant difference in trust and corruption levels between Ottoman and non-Ottoman parts of the non-Habsburg control group (not shown). Consequently, we cannot reject the interpretation that our results are homogeneous across neighboring empires, although they are most cleanly identified in the comparison to previously Ottoman areas.

Note also that substantial parts of our Habsburg treatment group at some stage in history had also been part of the Ottoman Empire. In fact, also among the observations in our border sample that had ever

been part of the Habsburg Empire, nearly three quarters had also at some stage been part of the Ottoman Empire, making Ottoman influence something that hardly differentiates the Habsburg treatment and control groups in our analysis. To rule out that our results capture an “Ottoman effect” rather than a Habsburg effect,<sup>42</sup> it is thus possible to restrict the whole analysis to a sample of areas that have all at some stage been part of the Ottoman Empire. One part of this sample has been part of the Habsburg Empire afterwards, the other part has not.<sup>43</sup> As reported in the bottom panel of Table A.6, our results are robust also in this specification (with the trust results coming out even more clearly in the border specification but shy of statistical significance in the RDD specification).

In an alternative specification to account for Ottoman influence (not shown), we include in the full sample an indicator for having ever been part of the Ottoman Empire as an additional “treatment” indicator alongside the indicator for ever having been part of the Habsburg Empire. The Ottoman indicator is never significant, and the Habsburg effect remains robust (although statistical significance becomes marginal for some outcomes). Our results thus reflect an influence of Habsburg affiliation (relative to lack of Habsburg affiliation), and no similar results are given for Ottoman affiliation (relative to lack of Ottoman affiliation).

These results indicate that in our analyses, the Habsburg effect is mainly identified from a comparison with control locations that were part of the Ottoman Empire, but also that they capture a genuine Habsburg effect rather than an Ottoman effect.

## *B.2 Duration of Affiliation with the Habsburg Empire*

A second dimension of possible heterogeneity relates to the duration of affiliation with the Habsburg

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<sup>42</sup> For example, Grosjean (2011b) finds a significant negative association of Ottoman rule with financial development, but not with income or enterprise development, in South Eastern Europe.

<sup>43</sup> While many regions changed from Ottoman to Habsburg rule over history, in our sample there are no incidents of changes in the opposite direction.

Empire. To test this, we estimate models that allow the Habsburg effect to vary with the duration of a community's affiliation with the Habsburg Empire:

$$y_{ilc} = \alpha_c + \beta_1 H_{ilc} + \beta_2 D_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc} \quad \text{if } i \in \text{border region} \quad (\text{A.1})$$

where  $D_{ilc}$  is the duration of the Habsburg affiliation of the community in which individual  $i$  lives.

Table A.7 shows both the binary main Habsburg variable  $H$  and the duration variable  $D$ , which is centered at 100 years (close to the average Habsburg affiliation in our border sample, see Table 1). The coefficient on the main Habsburg dummy  $H$  can thus be interpreted as the effect of being affiliated with the Habsburg Empire for 100 years. The coefficient on the duration variable  $D$  measures whether differing duration of affiliation changes the impact on the outcomes. It turns out that, with one exception, the duration effect is statistically insignificant. In the exception – trust in courts – the duration estimate is actually negative, but this also loses significance once the Sanjak area (see below) is dropped from the analysis.

This simple duration analysis masks important specifics, though. In particular, there are three areas that had been part of the Habsburg Empire at some time in history but were no longer in 1900 (see Figure 1). Silesia in Poland, which belonged to the Habsburg Empire from 1526-1742; the Kingdom of Serbia and the Banat of Craiova in Romania, 1718-1739; and West Galicia, which came to Habsburg in the Third Partition of Poland in 1795 but was lost again in 1809. While limited sample sizes prevent specific analyses in these areas, the specifications reported in Table 11 in Becker *et al.* (2011) drop the areas individually and jointly, to check that these historical affiliations are not solely driving our results. Qualitative results are robust in the sample that drops these three areas from the analysis, although the effect on trust in courts loses statistical significance. This last specification is effectively an analysis of the effect of having been part of the Habsburg Empire in 1900, rather than the definition used in our main analysis of having ever been part of the Habsburg Empire.

Beyond these specific areas that capture variation in the termination of Habsburg affiliation, there is basically no within-country variation in our border sample in the start and thus duration of Habsburg affiliation. Basically all remaining locations in the border sample came to the Habsburg Empire during the 18<sup>th</sup> century. The one exception is the Sanjak, an area on both sides of the border between Serbia and Montenegro where Habsburg troops were stationed nearly to the end (1908), but only since 1878. As the last row of Table 11 in Becker *et al.* (2011) shows, the trust results lose significance in the sample without the Sanjak, whereas the corruption results are robust.

Given this limited and specific variation in duration of Empire affiliation, the analyses of duration effects in our application are limited and do not allow strong conclusions on the validity of different hypotheses of how changes in trust and corruption come about and persist. However, the presented results do not provide evidence in favor of the hypothesis that the strength of the Habsburg effect *increases* with duration of affiliation with the Habsburg Empire. Rather, they are consistent with an interpretation that there is an effect of ever having been exposed to Habsburg rule, rather than of the length of exposure (thereby also supporting our main specification that defines Habsburg affiliation as having ever been part of the Habsburg Empire). This finding contrasts with descriptive evidence in Grosjean (2011a) that only history of several centuries of common empire exerts cultural effects. The absence of a duration effect is consistent with theoretical models such as Guiso *et al.* (2008b) in which a long-term equilibrium can be tipped by short-term exposure.

### B.3 Trust in Central Public Services

An open question is whether Habsburg raised individuals' trust in the state only at the local level or are generally also at the central level. It seems more likely that trust in the state survived for local public services, where the conduct and execution could have remained different despite the same formal legal rules. The courts and the police may well function differently in different parts of the country. By



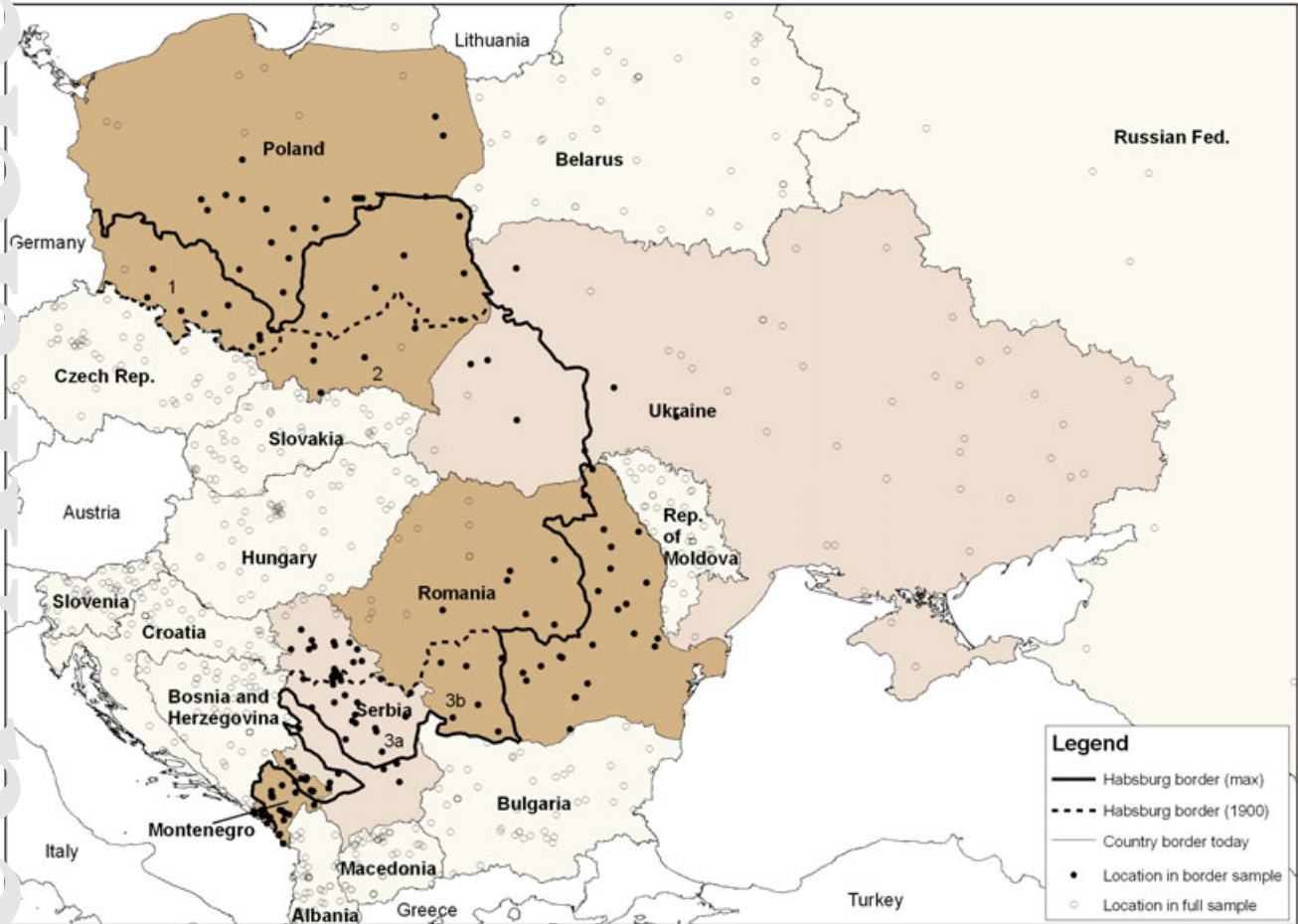
contrast, at the level of central public institutions, there is only one parliament, one president, etc., at least for the within-country variation exploited in our border sample. However, differential trust in central state institutions may still have survived even though centralized state institutions have been the same for former Habsburg and non-Habsburg areas for a long time, for example because individuals may at least partly extrapolate their local experiences with the state to the central level, because individuals on the Habsburg side may perceive the same central state agencies differently, or because of actual local differences in the impact of policies implemented by central authorities.

In separate analyses, we analyze the effect of Habsburg affiliation on the levels of trust in a number of central public services: political parties, the parliament, the government, the presidency, and the armed forces (see Table 12 in Becker *et al.*, 2011). While all coefficients are positive, none of them reaches statistical significance at conventional levels, and most point estimates are quite low.<sup>44</sup> One might be tempted to read these results as another falsification test for our results on local public services, which in total are much stronger. However, given that all point estimates for central public services are positive and quantitatively relevant effects cannot always be rejected, another plausible reading is that individuals on the Habsburg side indeed extrapolated at least part of their positive experience with local public services to trust in state institutions more generally.

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<sup>44</sup> Similarly, the LiTS question on bribes is also surveyed with respect to requests for official documents (like passports), public education, unemployment benefits, and other social security benefits. All point estimates on the Habsburg indicator are negative but do not reach statistical significance.

Figure 1: The Habsburg Empire in Eastern Europe and the LiTS Locations



Habsburg border in maximum expansion, Habsburg border in 1900, borders of countries today, and location of the observations in the LiTS 2006 dataset contained in the border sample and in the 17-country sample. Former Habsburg territories that are no longer part of Habsburg in 1900: 1 Silesia (1526-1742); 2 West Galicia (1795-1809); 3a Kingdom of Serbia and 3b Banat of Craiova (1718-1739).